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Encountering Alan Wilson, filling the gaps in data sets (from the knowns to the unknowns) ... and disciplinary history/progress¹

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This essay outlines my interactions with Alan Wilson over more than four decades, focusing on my early work on gravity models and then my adoption of his entropy-maximizing procedure in the study of disaggregated voting patterns. Using that example, the essay explores its implications for the study of disciplinary history.

KEYWORDS Alan Wilson, gravity models, entropy-maximising, voting patterns, disciplinary history

Introduction

I first ‘encountered’ Alan Wilson in 1969: I was in the UK from New Zealand for a conference and visiting my parents in Leeds; my head of department suggested I call into the university to see Bill Birch, recently-appointed as head of the department of geography there (Butlin 2015), and he told me of Alan Wilson’s appointment to a chair. Four years later I was on study leave in London and again visited Leeds, meeting Alan for the first time when we discussed my then current research interest in gravity models. A few years later Alan invited me to become a co-editor of *Environment and Planning A* and, at about the same time, colleagues pointed me to the relevance of Alan’s work to a new research interest of mine, which I then discussed with Alan. We worked closely on the journal for some years and encountered each other on numerous occasions – including as university Vice-Chancellors (Alan for much longer than me) and as Fellows of the British Academy.

Reflecting on those encounters raised wider issues with me regarding how the history of our discipline – human geography – has unfolded in recent decades. Those issues are the focus of the final section of this essay.

Gravity models, spatial structures, and entropy

For my year’s leave from the University of Canterbury in 1972-1973 I decided to branch out from my then established research interests in urban geography. I had been intrigued by Brian Berry’s (1967) case for the study of cities as systems within systems of cities and decided to extend that argument to the international scale. I drafted a short book while teaching for a semester at the University of Toronto (Johnston 1973a; we also used that framework in Coates et al. 1977) and began to develop a research project studying trade flows as a spatial system to be completed during a six-month fellowship at the London School of Economics (which eventually became Johnston 1976a).

Before going to Toronto I started fitting gravity models to some specimen data sets and became intrigued by an issue that bothered me: it seemed that when fitted to flows from individual places the distance parameter of that model was in part at least a function of the

¹ My thanks to Kelvyn Jones and Charles Pattie for discussion of many points and comments on a draft version.

spatial structure of the places involved (my first explorations of the model were in Johnston 1970). I discussed this during regular Toronto lunchtime sessions with Les Curry (who had recently published a paper on the issue: Curry 1972), Ross McKinnon and Alan Scott, but was finding it hard to formulate. That remained the case a few months later when I met Alan Wilson: I explained the issue and he told me of the entropy-maximising models he was working on. I went back to London and read a copy of *Entropy in Urban and Regional Modelling* (Wilson 1970), which a recent review by Peter Gould (1972) had told me was the *most difficult* book in geography he had ever read.² I worked through both that review and Alan's book, gained a general appreciation of the method and its applications, and decided that it was not directly relevant to what I was doing.

My own concern followed directly on from statements by Gunnar Olsson (1970, 27 – after Rushton 1970) that ‘the determination of distance functions may in fact say more about the spatial distribution of opportunities than about spatial interaction per se’ and Les Curry's (1972, 132) argument that a non-zero exponent for the distance variable had ‘nothing to do with friction and everything to do with the map pattern’; any derived coefficient would be specific to the particular pattern of origins and destinations in the data set being analysed so that ‘different degrees of clustering will exhibit different frictional terms even if friction is known to be constant’. He further argued that Wilson's early work showed that the ‘difficulties of estimating [the gravity model's] ... parameters are virtually insuperable’ (p. 135).³ I found the rest of his paper fairly impenetrable, and pressed on with my own explorations, which were published (Johnston 1973b) about the time I arrived in Toronto. What I showed, using a simulated data set, was that the size of the distance-decay exponent for flows either to or from any individual place was strongly related to the spatial structure. Cliff et al. (1974, 281) responded to this paper, claiming that ‘except possibly for certain intra-urban situations, the expected value of the estimated regression coefficient for the distance exponent is not influenced by any map pattern in the spatial system being studied’, a conclusion they sustained by both simulated and real data sets. I contested that (Johnston 1975) because they were not addressing the same problem – my issue concerned the distance exponent when the gravity model is fitted separately for each place in the spatial system whereas they fitted it to all places – the single exponent that they found would differ from system to system depending on the pattern of the places studied, in my view, but they studied just one system. Curry et al. (1975, 295) then entered the debate, concluding from a re-assessment and re-analysis of Cliff et al.'s (1974 – CMO) argument that ‘although CMO were correct they did not answer the main question. While estimation may be unbiased, the distance coefficient being estimated in fact is a conglomeration of two effects, and what is crucial is to separate the distance effect due to spatial interaction from that due to spatial pattern’. Cliff et al. (1975) responded that when the distance-decay effect is invariant then spatial pattern is irrelevant – but that again avoided the main question, which concerned variations in that effect, as Sheppard et al. (1986) pointed out in a ‘final comment’.⁴ Meanwhile I extended my arguments by – following Peter Taylor's (1971a, 1971b) pioneering work on distance-decay functions⁵ – showing that my conclusions were invariant to the measure of distance deployed (Johnston 1976b).

² See Senior's (1979) exposition.

³ Gould (1975, 93) later similarly concluded that ‘the distance parameters appear to index the relative accessibility of a location’: I was somewhat disappointed that he didn't refer to my 1973 paper, especially as his co-author for much of his work on mental maps – Rodney White – was another Toronto colleague with whom I discussed the issue!

⁴ Interestingly, in neither that nor their earlier comment were my papers cited!

⁵ Those two pioneering papers didn't, in my view, receive the attention they deserved.

Sheppard (1979) continued exploring the issue and he (Sheppard 1984) and Griffith (2007) both provided later overviews of the debate, with the latter concluding that Curry's (1972) contention had been sustained; Fotheringham (1981, 1982) later reached similar conclusions, extending them by contrasting what he termed agglomeration and competition effects. But by then, for me, it had ended. Having completed the book on world trade my attention turned elsewhere. I had nagging feelings that the issue of spatial structure was also relevant to entropy-maximising exercises.⁶ As I understood it, in traffic flow applications the number of potential movements and the locations of the origins and destinations were given, as was the network of routes linking them, and the gravity model theory that Wilson developed then introduced a global constraint – in effect the gross cost of all movements – within which all moves were to be contained. With x_i number of people moving from each origin and y_j arriving at each destination, c_{ij} being the cost of moving from i to j , and C being the total expenditure on all movements, the entropy-maximising procedures identified the most-likely pattern of flows – f_{ij} – that would meet those constraints. With a different spatial structure – i.e. the same number of i and j locations, but differently distributed, hence a different c_{ij} matrix – a different pattern of interactions might emerge. But I never explored it, nor raised it with others.

Estimating the unknowns from the knowns: entropy and voting

By the late 1970s my research interests, building on some early research undertaken before I left New Zealand and much stimulated by a decade of close collaboration with Peter Taylor, who had done pioneering and extremely innovative work in the area (e.g. Gudgin and Taylor 1979), had become largely focused on electoral studies.

One of the then dominant stylised facts derived from analyses of British general election results was that of uniform swing, a concept popularised by the doyen of British psephology, David Butler (cf. Butler and Stokes 1969). Several ways of measuring swing have been derived over the years, but a simple example will illustrate the basic argument. Two general elections are fought by two parties only. At the first, party A gets 70 per cent of the votes cast and party B gets 30 percent; at the second their relative shares are 64.5 and 35.5. Swing is then defined as the percentage of the voters who shift from one party to the other – which is 5.5 percentage points, in this case away from A and to B . (Where more than two parties are involved, two-party swing – involving the two largest parties – is calculated as half of the sum of the absolute values of one of those party's declining percentage share and the other's increase: see Curtice et al. 2016, 388ff.) The stylised fact was that the national swing applied everywhere – the same shift occurred in each constituency, with some relatively minor variation around the national figure (Butler and Stokes 1969). But, as Berrington (1965) pointed out, if there is a uniform swing, because constituencies vary in each party's relative strength this means that the uniform pattern is the result of geographically-varying changes.

Take the situation when there is a national swing of 2.5 percentage points away from A and towards B . Two constituencies – x and y – are shown in Table 1: x is a safe seat for A ; y is a marginal. In each, A 's share of the votes cast falls by 2.5 percentage points – from 70 to 67.5 in x and from 52 to 49.5 in y ; B 's shares increase by the compensating amount in each.

⁶ And, as I later became aware when the lead author of Bruna et al. (2016) visited us in Bristol, to other studies of spatial structure.

Because the total number of votes at each election in each constituency was 100,000, the number of voters changing party was 2,500 in each. But when these changes are expressed as a percentage of each party's vote total at the first contest, they vary – between constituencies within parties and between parties within constituencies. Party *A* lost 4.81 per cent of its support in *y*, but only 3.57 per cent in *x*.

With a uniform swing, therefore, you get the apparent paradox that – on the reasonable assumption that places (in this case British parliamentary constituencies) varied in their support at any election for the main political parties – the amount of change between elections would vary across constituencies (as I suggested in an early study of variations between 1974 and 1979 at the sub-regional scale: Johnston 1979). Uniformity of outcome on one measure of change was the cause of variation on another. Each constituency, therefore, would have its own inter-election voter transition matrix – an example of which is given in Table 2. This refers to a constituency contested by three parties (*A*, *B* and *C*) and with a number of others abstaining (*DNV*) at both election *i* and election *j*. The total numbers voting for each party at each election are known – the row and column sums – but the internal cell values (e.g. A_iA_j – the number who voted for party *A* at election *i* and remained loyal to it at party *j*) are unknown. How could those unknowns be estimated – in the absence of any data due to confidentiality restrictions and lack of linkage over time. Opinion polls and voter surveys could be used to determine the national transition matrix (i.e. the value of A_iA_j was known for the whole country with some accuracy), but those surveys were insufficiently large to provide estimates for each constituency; indeed, at that time the main voter surveys – the British Election Study – only sampled from a relatively small proportion of the country's constituencies.

Could a way be developed using those three sets of knowns – the vector of votes for each party at each election and the national voter transition matrix – to estimate the unknowns for each constituency? I discussed that on several occasions with my Sheffield colleague, Alan Hay, and he suggested that entropy-maximizing might be a viable estimating procedure. We then raised that with a colleague in the University of Sheffield's Department of Town and Country Planning, Ian Masser, who had worked on transportation models, and he confirmed Alan's view. And so we set to finding a way of doing it, using the published election result data plus a voter transition matrix obtained from the British Election Study surveys, kindly provided to us by John Curtice – producing a data cube with all its internal cells being unknowns. The problem was recast in entropy-maximizing terms, and the goal was to find the most likely set of transition matrices, one per constituency, that met the row, column and national matrix constraints. The way to do that was by deploying biproportionate matrix smoothing (Bacharach 1970; Bishop et al. 1975; Mosteller 1968: because we were analysing a data cube we called it triproportionate smoothing) – for which I wrote a FORTRAN program whose operations on a contrived data set were illustrated in a first paper (Johnston and Hay 1982; a second introduced the method to the political science community: Johnston and Hay 1983). The method was then applied to British (Johnston 1982a, 1983c) and New Zealand data (Johnston 1983a),⁷ with analyses of the outcomes identifying reasons for the spatial variations in the estimated transition matrix cells – such as type of constituency, each party's constituency vote at the first of the two elections, and the intensity of the parties' local campaigning there.

⁷ Alan Hay decided that as the study of elections wasn't his research interest he would not be involved in the papers where I applied the method, only in those that presented its technical features. Clearly without his and Ian Masser's original input, those subsequent papers could not have been written.

Our estimates indicated the extent of the likely spatial variation in voter transition matrices across the UK: indeed, these were possibly conservative estimates, given the known constraints of the actual election results in each constituency. Thus, for example, in an early study using the two general elections of 1974, whereas nationally some 74 per cent of those who voted Conservative at the February election did so again in October, the entropy-maximizing estimates ranged across constituencies from 52 to 82 per cent (Johnston 1982a). In a four-way table – voting for three parties (Conservative, Labour and Liberal) plus Non-Voting – the coefficient of variation around each of the sixteen cell averages ranged from 0.037 (voted Labour at both elections) to 0.309 (voting Labour at the first contest and Liberal at the second), with a mean of 0.165. Explanations were then sought for those variations; one important influence was the amount spent by a party on the constituency campaign – the more that a party spent, for example, the more of its supporters at the first contest who remained loyal at the second.

As well as resolving the uniform swing paradox we also realised that the procedure could be used to address another stylised fact – that the class cleavage which characterised British politics was uniform across the country: e.g. if 64 per cent of middle class voters supported the Conservative party nationally the same percentage, presumably with a small error term, would do so in every constituency (Bogdanor 1983).⁸ Since the British Election Study survey data gave the pattern of voting by class at every election, the census gave the class breakdown of the working population in every constituency, and we knew the number of votes cast in each constituency, again we had a data cube of knowns and unknowns and the entropy maximizing procedure was deployed in an initial study of the 1966 British general election (Johnston 1982b), which showed that the uniformity of voting stylised fact didn't apply: the more middle class voters in a constituency, for example, the more middle class voters there who voted Conservative – a pattern consistent with the widely-discussed neighbourhood effect hypothesis of voting patterns (introduced to geographers by Cox 1969; see also Miller 1977; Johnston 1983b). For example, according to national survey data, in 1983 51.7 per cent of the middle class voted Conservative, but that varied across English constituencies from 14 to 63, with a coefficient of variation of 0.21. There was an even greater variation in the middle class percentage voting Labour – from just 1 to 32, with a mean of 11.6 and a coefficient of variation of 0.60.

The data being analysed in these papers – used as the dependent variables in testing hypotheses regarding spatial variations in voting behaviour – were only estimates, albeit the 'best estimates' derived from a proven mathematical procedure. How accurate were they; were there any data against which we could test their validity? An Australian geographer, Denis Rumley, had conducted a large sample survey of voters in several Western Australian constituencies and we approached him to see if we could use his data to evaluate our procedure. Dividing voters into just two social classes and establishing their support for the two main political parties/coalitions contesting the election, we estimated the percentage in each class in each of the ten constituencies who voted for each party. For working class voting for the ALP this ranged according to our estimates from 23.8 to 66.9 per cent, whereas in the survey data the range was 24.2-70.8: the mean absolute difference between the estimated and actual values was 2.4 percentage points (Johnston, Hay and Rumley 1983) – a finding which gave us considerable confidence that the method was viable. Given that

⁸ My quotation from Bogdanor's book in a review article (Johnston 1986) stimulated a claim from him – which I refuted – that I had misquoted him (Bogdanor and Johnston 1986).

conclusion we then extended the analysis to all 55 Western Australian constituencies at the February 1977 state election there, and successfully tested hypotheses regarding spatial variations in the estimated values (Johnston, Hay and Rumley 1984).

Having established the method, I applied it in a range of contexts. By then I was working closely with Alan Wilson, who in 1979 had invited me to become a co-editor of a journal that he had founded a decade earlier – *Environment and Planning* (later *Environment and Planning A*). At one of our editorial meetings I discussed what Alan Hay and I were doing, and he not only offered encouragement but also helped me to develop a clearer understanding of the underlying mathematics, which I set out in an appendix to a book on the 1983 general election (Johnston 1985). Alan Hay and I extended the range of applications to a study of ticket-splitting at an American election (Johnston and Hay 1984) and with Peter Taylor we essayed a further extension to incorporate migration (Johnston, Hay and Taylor 1982).

The 1987 British general election provided a further context within which to deploy the procedure. By then I was working closely with Charles Pattie, and we used estimates of change between 1979 and 1987 to evaluate the contemporary argument regarding a ‘nation dividing’ (Johnston and Pattie 1987), analyses that were extended into a book-length study with Sheffield’s cartographer, Graham Allsopp (Johnston et al. 1988). Charles and I then got a grant from the ESRC to extend the work (e.g. Johnston and Pattie 1988; Johnston, Pattie and Johnston 1988) and explore further aspects of the method – about which only one paper appeared, on measurement and sampling error (Johnston and Pattie 1991). The method was introduced to wider audiences (Johnston and Pattie 1992, 1993, 2000, 2001); one of those papers (Johnston and Pattie 1999) applied it to a new context – split-ticket voting in New Zealand’s multi-member proportional electoral system, an application later extended to the first use of that method in elections to the Scottish parliament and the Welsh Assembly (Johnston and Pattie 2002). We approached Thomas Gschwend, who had been studying split-ticket voting in Germany, and an application to his data allowed an evaluation of spatial variations there (Gschwend, Johnston and Pattie 2003). A visit to Mannheim led to a collaboration with Thomas and Martin Elff in which we extended the method to incorporate error – based on a parallel between entropy-maximising and log-linear modelling, which allowed us to add error terms to estimates of split-ticket voting at New Zealand’s 1996 election (Elff, Gschwend and Johnston 2008).

This substantial body of work undertaken over three decades clearly illustrated that Alan Wilson’s approach to modelling data – in effect, estimating unknown data from known constraints – had wider applications than those on which he and his co-workers at Leeds had focused. As far as we could find, only one other type of application had been undertaken, in studies of migration matrices by, for example, Chilton and Poet (1973), Willekens (1977) and, much later, Dennett and Wilson (2013); Willekens (1994) reviews that literature. (Alan Wilson’s own – 2010 – retrospective essay reaches the same, implicit, conclusion.) One problem may have been with terminology. For example, Wong (1992) argued that geographers had largely ignored the biproportional iterative fitting procedure advanced by Bacharach (1970) for generating individual from aggregated data but this, as we pointed out (Johnston and Pattie 1993), was exactly the procedure we had used in our voting studies (an alternative term was ‘Mostellerisation’: Mosteller 1968; Särnkvik and Crewe 1983), and we pressed its wider application. But our applications continued to go largely unrecognised among geographers: a decade later, a set of papers in the *Annals of the Association of*

American Geographers (Sui 2000) failed to recognise that entropy-maximising had been used to resolve the ecological inference problem in voting studies (Johnston and Pattie 2001).

Indeed, in voting studies although the method and its use were widely published in a range of journals – with the method clearly being accepted by referees – nevertheless we were never asked either for code which others could apply in their own work or to collaborate by running our program on colleagues’ data; only one researcher (Berg 1988: see also Füle 1994, 1995) applied it using his own bespoke software (see also Sepúlveda and Bengoechea 2018). Others had sought procedures by which the ecological inference problem could be resolved through estimating the unknowns in transition matrices (e.g. Brown and Payne 1986) and their detailed review of several methods included a careful evaluation of our entropy-maximizing procedure (Cleave et al. 1995; they argued that the procedure was not strictly an attempt to resolve the ecological inference problem since it deployed individual level data – the survey data used as the national transition matrix estimate – as well as aggregates). Soon after the appearance of that review and critique Gary King published his *A Solution to the Ecological Inference Problem* (1997), which gained very substantial publicity, has garnered over 1,100 citations according to Google Scholar⁹ and, although subject to considerable critical evaluation (e.g. Freedman et al. 1988, 1522 – ‘In short, King’s method is not a solution to the ecological inference problem’; see the defence in Voss 2004), it has become widely used and alternative methods increasingly ignored (e.g. Cho and Manski 2008).⁹

Nor was there any reciprocal relationship between our work using entropy-maximizing to estimate the values of the internal cells of large cross-classifications and developments in the modelling undertaken at Leeds. In 1976 Wilson and Pownall (1976) addressed the issue that entropy-maximizing shopping models, which addressed the flows between residential zones and shopping areas, did not provide estimates of the number of people who, for example, lived in one zone, worked in another and shopped in a third, and they presented an algebra for doing that. This was similar to the method of micro-simulation widely used by economists and others to, for example, evaluate the impact of public policies, such as changes in taxation regimes. This was taken up and deployed in a wide range of contexts (see Ballas et al. 2005; Clarke 2006), such as estimating unknown values in census data. The approach, and its outputs, have many similarities to that deployed in our voting studies – as suggested in, for example, Birkin and Clarke (1988, 2009), Birkin et al. (2010) – but the links were never made, by either group.

We have some data, but how to make them more reliable?

In all of these applications, the goal has been to provide estimates of unknown values in an n-dimensional data matrix. In considerable part this was a reflection of the times and the relative paucity of data which made estimating a necessity. Several decades on this is not such a problem. In electoral studies, for example, the growth of survey and polling operations using the internet means that many of the cells previously unfilled can now be at least partially filled. With large matrices, however, the number of observations in at least some cells may nevertheless be small and the calculation of ratios (such as the percentage of a constituency’s residents who voted for a particular party at an election) subject to considerable error. For that reason, it is desirable to model the pattern across all cells to get more reliable estimates, with their Bayesian credible intervals. Such a strategy has recently

⁹ King published his software: <https://dash.harvard.edu/handle/1/3965179> - accessed 28 March 2018.

been developed (Jones et al. 2016) and applied to British electoral data (Johnston et al. 2018). It remains to be seen whether this attracts more attention than the entropy-maximizing method! Comparable work has been undertaken in the polling industry, however, seeking estimates of an election outcome, prior to its conduct, not only nationally but also in every constituency. This uses Multiple Regression and Post-Stratification (MRP) to derive those estimates, combining polling with various aggregate datasets (Gelman and Little 1997; Gelman et al. 2017; Ghitza and Gelman 2013; Hanretty et al. 2018 – see also Johnston et al. 2018) – and a polling company was established in 2018 which is explicitly focused on using that method.¹⁰

On scientific progress

Science is a cumulative activity, with each round of activity building on the results of those proceeding it. At its core are its publications, the scientific papers and monographs that report new findings and methodologies and the textbooks that bring those materials together to represent the current state of knowledge in a field or, increasingly, sub-field, even sub-sub-field.

Given the depth and breadth of contemporary science, most researchers operate in just a small portion of it only: they are able to keep pace with and contribute to only a small proportion of the published literature and can sustain at best only a general interest in and appreciation of what is being done even in adjacent fields. Modern science is largely performed in nested silos: most of the larger units are named after disciplines – geography, say – or major divisions within them – human geography – but few practitioners operate across the full breadth of their macro-silo's content. Each of those macro-silos is divided into a number of meso-silos and they in turn into even more micro-silos – some of which cross disciplinary boundaries. Some researchers are able to operate in more than one micro-silo – they may have to in order to sustain up-to-date teaching portfolios (as exemplified by general textbooks) – but research specialism, indeed micro-specialism, is the norm.

Each micro-silo has a social network of practitioners who interact both inter-personally – at conferences, seminars etc. – and indirectly through their publications and other communications, together creating what have become known, after Latour (2005), as actor-networks. The ties within those networks tend to be relatively strong, although some individuals may participate less than others, and their progress is internally assessed. But unless the silo boundaries are to some extent porous, the work that is undertaken within them will be isolated from developments elsewhere. There is the need for inter-silo interaction, for what Granovetter (1973) terms weak ties; links from one network to another – perhaps personal, perhaps by somebody in one network reading and appreciating something published in another – ensure an openness to new ideas and methods developed elsewhere. (There are links here to the arguments regarding the 'wisdom of crowds': Surowiecki 2004.)

But are those weak ties sufficient? Are the necessarily specialised micro-silo (even meso-silo) networks open enough to others through weak ties that cross-fertilisation occurs, that developments in one influence others? Clearly in some cases yes – as in the case discussed here of our application of entropy modelling to the study of voting patterns. But are there enough of them? I doubt it.

¹⁰ Deltapoll - <http://www.deltapoll.co.uk/>; <https://www.research-live.com/article/news/boon-flatters-and-twyman-launch-deltapoll/id/5035612>.

Take the example of Alan Wilson's development of urban and regional models from the 1960s on. As he sets out in his retrospective essay (Wilson 2009), a great deal was done by him and his colleagues building on that foundation. They also used it commercially, in their successful GMap enterprise which applied those models to seek optimal locations for a range of commercial activities (Birkin et al. 1996). But what of any wider influence, beyond the network of geographers and planners that he stimulated and operated at the centre of for several decades? Of this there is little evidence. Although its relevance to, and importance within, geography has been challenged, the use of quantitative methods and mathematical modelling is a substantial component of the discipline (Johnston et al. 2014). Textbooks presenting those methods to undergraduates are produced on a regular, if not frequent, basis and there are calls from outwith as well as within the discipline for the improvement of technical skills. But entropy modelling and all that it has spawned appears rarely in such volumes: by implication, because authors rarely say why they have omitted things, it is considered too specialist, marginal and, probably, difficult to be included in the basic courses that the textbooks are aimed at. (Taylor's, 1977, book is one of the few that addresses Wilson's work, albeit relatively briefly. It gets more detailed coverage in the second edition of *Locational Analysis in Human Geography* – Haggett et al. 1977; by then Haggett had fully accepted the argument advanced by his co-author Cliff that because of the spatial autocorrelation issue the general linear model should not normally be applied to geographical data.) Nor is there much evidence that it has influenced workers in other macro-silos: Dempster et al. (1977), for example, presented an algorithm for 'computing maximum likelihood estimates from incomplete data', now used in major software packages such as MLwiN, with no reference to Wilson's, or any similar, work. (Intriguingly, one of the discussants of that paper – J. A. Nelder – began his remarks by noting (p.23) that 'our subject is becoming more and more fragmented with people quarrying vigorously in smaller and smaller corners and becoming more and more isolated from each other in consequence'.)

That the method had wider potential is illustrated by my own adoption of it – thanks to the weak ties provided by Alan Hay (a transport geographer), Ian Masser (a planner) and Alan Wilson himself, without whom I would almost certainly never have made the link. Indeed, the discussion of my work on gravity models shows that while aware of the method I had not conceived of its potential and when, a decade later, I was doing work that it was relevant to I did not make the link. But either there were no other potential links, which I doubt, or there were no weak ties that enabled them to be made – perhaps Alan and his many co-workers stayed too much within their own silo and when they tried to make links, through textbooks, for example (such as Wilson 1981), and plenary lectures (Wilson 1972), they failed to find a receptive audience.

This situation is far from unique to this one case study: indeed, my own experience with the application of entropy modelling to voting studies is yet another example. I became interested in electoral matters while still in New Zealand in the late 1960s when stimulated by an essay on the geography of voting to test some hypotheses with local data and, while doing that, came across a paper by a New Zealand political scientist that led me into another topic within electoral studies (Brookes 1960).¹¹ After my move to Sheffield in 1974 elections

¹¹ That provide a further example of the general argument being made here. I adopted and, with David Rossiter, Charles Pattie, Michael Thrasher, Collin Rallings and Galina Borisjuk, adapted Brooke's method for identifying bias in the operation of electoral systems, that we have widely applied in the UK over the last four decades (e.g. Johnston et al. 2001). A version of that method was later developed in the United States (e.g. Gelman and King 1994) and deployed by them and others in challenges to gerrymandering that went to the Supreme Court but, although we had applied our

became the main focus of my research activities. There was a very small community of electoral geographers in the UK (the community of political geographers was not large and for them electoral studies were of marginal interest) and so I made the decision to participate in the activities of political scientists – who established, and invited me to join from the outset, an Elections, Public Opinion and Parties specialist group in the early 1990s.¹² I published widely in politics journals, including a number of papers using the entropy-maximising methodology (discussed above) but, although those papers passed the peer review processes and the findings using the method were never questioned, nevertheless – as detailed earlier – it was never adopted by others.

In a seminal, though not widely cited, paper¹³ Simon Duncan (1974) explored why what became an important innovation in geographical analysis by a Swedish geographer had a delayed reception. He suggested three possible reasons why a piece or body of work might be (relatively if not absolutely) overlooked: (1) general social resistance, perhaps linked to linguistic or political barriers; (2) a ‘paradigmatic effect’, with the work being largely ignored because it did not fit into the discipline’s established practices; and (3) a ‘Matthew effect’ – science is practised in interacting communities, groups of individuals who share common interests (research subjects, methods etc.), and the work of outsiders – especially those with few if any links to one or more of those communities – may be ignored because of the lack of contacts. Of these, the first is not directly relevant – all of the work discussed here by Alan Wilson and his colleagues, and by me and my colleagues – was published in English, although the language of mathematics may have been off-putting to many lacking any post-school training in the discipline. But in my case there was a barrier – disciplinary. Despite the links that I built and the networks I joined I was (and still am) a geographer. Laplace (1980) noted some decades ago that geography had fewer links to the other social sciences than they had to each other, and this remains the case: relatively few political scientists, sociologists and economists cite geographical works, for example – and they probably rarely search the discipline’s literature. There are very few weak ties into other disciplines from geography – in this case, into political science, especially American political science.¹⁴

Nor does Duncan’s paradigmatic effect offer too much by way of explanation for the relatively limited engagement with which the two bodies of work using entropy modelling

version to American examples (e.g. Johnston et al. 2005) and I had made presentations at conferences and seminars there, it received virtually no recognition and the Gelman-King method is recognised as ‘the established methodology for measuring partisan bias’ (McGann et al. 2016, 56: to be absolutely fair, McGann et al. do refer in passing to two of our papers and Grofman, 1983, referred to it – and the Brookes papers – in the earliest American piece on what they term partisan symmetry, but in none of his later work; senior American political scientists who work on gerrymandering whom I met in 2018 were unaware of that substantial parallel strand of work).

¹² All four PhD students I supervised who did quantitative electoral research and have entered academic life – Charles Pattie, Andrew Russell, Edward Fieldhouse and David Cutts – now occupy chairs in British university politics departments.

¹³ Just 33 citations according to Google Scholar®.

¹⁴ Two books by American authors published by Cambridge University Press in 2017 illustrate this. Both have geography in their subtitles – *Red Fighting Blue: How Geography and Electoral Rules Polarize American Politics* (Hopkins 2017) and *The Space Between Us: Social Geography and Politics* (Enos 2017) – but to them the study of geography does not embrace what geographers do. Hopkins does not have a single reference to a work published by an academic geographer; Enos has only five (Johnston 2018). A more recent book all about place and geography Acharya et al. 2018) similarly does not embrace what geographers do – is that their fault, or geographers’, or just typical of the poverty of inter-disciplinary interaction?

discussed here – or mathematical modelling more widely – outwith the particular areas in which it was being deployed. Wilson introduced it when human geography’s ‘quantitative revolution’ was taking off and quantitative methods were being widely adopted and taught; there was no reason for it to be rejected because it ‘didn’t fit’ although, as noted above, most of the quantitative work being done then, especially in human geography, and since was statistical rather than mathematical so the fit was partial only (Johnston and Sidaway 2016). Furthermore, and perhaps importantly, whereas a large number of individuals was appointed to British university geography departments in the 1960s-1970s to teach quantitative methods (many of them Cambridge graduates) very few trained by Alan Wilson at Leeds were similarly appointed, other than those who remained there.¹⁵ Whereas most geography departments had embraced statistical analysis by the end of the 1960s few did so for mathematical modelling (Robson 1970, reports that 27 departments had a total of 44 staff involved in quantitative teaching in 1964, whereas in 1969 122 staff were so involved in 28 departments). Further, there is little evidence (despite recognition in his textbook on mathematics and geography: Wilson and Kirkby 1980) that Wilson and his colleagues interacted with work that addressed similar issues through statistical rather than mathematical calibration – such as work on estimating gravity model parameters using Poisson regression (e.g. Flowerdew and Aitkin 1982; Guy 1987; Baxter 1982; Batty and Mackie 1982). Similarly, there is no recognition that the entropy-maximizing approach used in our electoral studies could, as set out in Johnston (1985, 324ff., following Upton 1978; Upton and Fingleton 1989) could be restated as a log-linear model.

Which leaves the ‘Matthew effect’. As already discussed, most scientists – including most geographers of whatever adjectival persuasions – work within relatively-closed communities, or silos. Many, of course, have some external links, if only through their participation in university department teaching and other activities, but relatively few of these, it seems – at least from the examples deployed here – develop into (even temporary) weak ties whereby there is cross-fertilisation of ideas from one community to another. Occasionally individuals seek inspiration elsewhere when tackling a problem that seems irresolvable with their current methodological tool-bag (as I did when looking at the geography of voting); alternatively reading something outside one’s normal academic literature span, or hearing a visitor give a talk, stimulates a link that otherwise may never have been forged (or simply talking to a colleague about a research problem over coffee, as I did with Alan Hay).¹⁶ But for much of the time most of us operate within our relatively closed micro-communities (Geertz, 1983, compared them to villages) and because of that, as illustrated here, science progresses in a particular staggered way (Johnston and Jones 2019). Could it be any different?

¹⁵ Martyn Senior was appointed to the geography department at Salford in the mid-1970s but later moved to the planning department at Cardiff (see Senior 1979); John Beaumont spent a short period in geography at Keele before moving to business schools. Paul Williamson joined, and has remained at, the Department of Geography at the University of Liverpool.

¹⁶ On a flight to Seattle in 2006 I read *Freakonomics* (Levitt and Dubner 2005): one of its chapters stimulated a hypothesis that we hadn’t previously encountered in our work on campaign spending in the UK; on my return I found we had the needed data to hand, we tested the hypothesis, and the paper appeared later that year (Johnston and Pattie 2006).

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Table 1. An example of inter-election swing

Party	Election 1		Election 2		Change	
	Votes	%	Votes	%	Votes	%
<i>Constituency x</i>						
A	70,000	70	67,500	67.5	-2,500	-3.57
B	30,000	30	32,500	32.5	+2,500	+8.33
<i>Constituency y</i>						
A	52,000	52	49,500	49.5	-2,500	-4.81
B	48,000	48	50,500	50.5	+2,500	+5.21

Table 2. An inter-election transition matrix

Party	A_j	B_j	C_j	DNV_j	Σ_i
A_i	A_iA_j	A_iB_j	A_iC_j	A_iDNV_j	A_{ij}
B_i	B_iA_j	B_iB_j	B_iC_j	B_iDNV_j	B_{ij}
C_i	C_iA_j	C_iB_j	C_iC_j	C_iDNV_j	C_{ij}
DNV_i	DNV_iA_j	DNV_iB_j	DNV_iC_j	DNV_iDNV_j	DNV_{ij}
Σ_j	A_{ji}	B_{ji}	C_{ji}	DNV_{ji}	